REPLICA DI KRUPP

ALLA

PROTESTA

DEL

SIGNOR BASHFORTH

TRANSLATED WITH NOTES

BY

FRANCIS BASHFORTH, B.D.

FORMERLY FELLOW OF ST JOHN'S COLLEGE, CAMBRIDGE, AND
LATE PROFESSOR OF APPLIED MATHEMATICS TO THE ADVANCED CLASS OF
ROYAL ARTILLERY OFFICERS, WOOLWICH.

CAMBRIDGE:
PRINTED AT THE UNIVERSITY PRESS.
1898

Price One Shilling.

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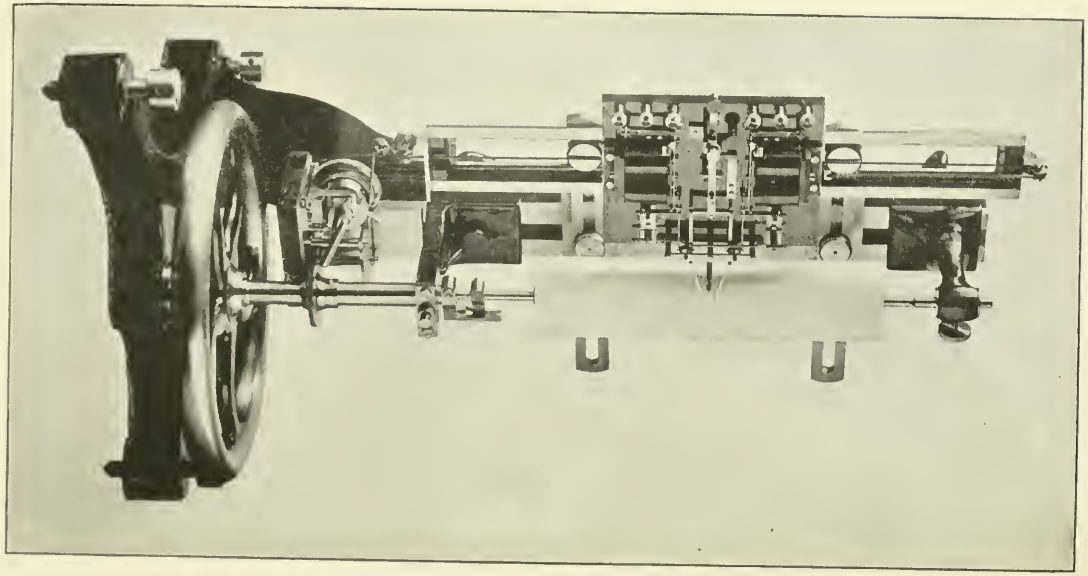

BASHFORTH CHRONOGRAPH,

Constructed for the Governments of France and Spain by Messrs Elliott Brothers.

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PREFACE.

Immediately after my appointment to the post of Professor of Applied Mathematics to the Advanced Class of R.A. Officers and Referee of the Ordnance Select Committee, 1864, I turned my attention to the Experimental determination of the Resistance of the Air to the Motion of Projectiles. Notwithstanding much official opposition, I succeeded in making all the desired experiments, so far as the power of the then existing Service guns permitted, that is, for velocities 900—1700 f.s.; and the Report of these experiments, 1868, was published in 1870. About ten years afterwards additional experiments were carried out for velocities 430—2250 f.s., 1879; and finally for velocities 100—2800 f.s., 1880.

In 1872 General Mayevski made free use of my results published 1868, as explained in his own words (22), naming *my* results "*Russian* and English" and ever after suppressing my own name. I fear that some of my good friends have fallen into this Russian snare when referring to *my own* results.

In 1881 the Krupp Firm issued in America close imitations of my General Tables 1871, 2, in both French and German, as discoveries of their own, not even once mentioning my name. As soon as I could obtain a copy of these precious documents I issued my Protest (28) against the proceedings of the Krupp Firm. A copy of this Protest was given in "Rivista di Artiglieria e Genio" and Mr Fried. Krupp replied in defence of his firm. I have to thank Colonel Siacci, for having very kindly sent me copies of Mr Fried. Krupp's "Replica," and also of the Editorial Remarks thereupon. I first became acquainted with these matters early this year, 1898. I therefore now give a translation of them, thus, at the same time, presenting Mr Fried. Krupp's view of the matter, and affording ample justification of my own Protest.

Minting Vicarage,
Sept. 1898.

1. Reports on Experiments made with the Bashforth Chronograph to determine the Resistance of the Air to the Motion of Projectiles, 1865–1870. Pp. 170. London, 1*s*.
2. Report on Experiments made with the Bashforth Chronograph to determine the Resistance of the Air to the Motion of Elongated Projectiles (Part II), 1878–79. Pp. 58. London, 1879. H.M. Stationery Office.
3. Final Report of Experiments made with the Bashforth Chronograph to determine the Resistance of the Air to the Motion of Elongated Projectiles, 1878–1880. Pp. 68. London, 1880. 2*s*. 3*d*.
4. Tables of Remaining Velocity, Time of Flight and Energy of Various Projectiles calculated from the Results of Experiments made with the Bashforth Chronograph, 1865–1870. Pp. 48. London, 1871.
5. A Mathematical Treatise on the Motion of Projectiles founded chiefly on the Results of Experiments made with the Author's Chronograph. By F. Bashforth. Pp. xxii + 90 + 112. London, 1873.
6. Supplement to the above. Pp. 42 + 80. London, 1881.
7. A Revised Account of the Experiments made with the Bashforth Chronograph to find the Resistance of the Air to the Motion of Projectiles, with the application of the Results to the Calculation of Trajectories according to J. Bernoulli's method by F. Bashforth. Pp. x + 318. Cambridge, 1890.
8. A supplement to the above, containing a historical sketch of the Progress of Ballistic Experiments connected with the Advanced Class, Woolwich, 1864–1890. Pp. viii + 56. Cambridge, 1895.

INTRODUCTORY REMARKS.

1. Shortly after sending in my "Final Report" on experiments made to determine the resistance of the Air to the motion of Elongated Projectiles, 1880, accompanied by General Tables connecting velocity and time, and velocity and space, for velocities 100—2800 f.s. or 30—853 m.s., information reached me that the Krupp Firm was issuing similar tables in the United States, America. I was not able to obtain a sight of this work until December, 1883, when a copy reached me viâ America and Woolwich. The title was "Krupp'sche Tabelle zur Berechnung der horizontalen Endgeschwindigkeiten und der Flugzeiten der Langgeschosse: Essen, 1881." There was also a French Edition of this work: "Table de Krupp pour le calcul des vitesses restantes horizontales et des durées de trajet des projectiles oblongs: Essen, 1881," for velocities 140—700 m.s., or 459—2297 f.s.

2. This work was found to be merely a reproduction of my General Tables, 1871, 2 and 9, using French measures and supposing a slightly reduced resistance to adapt my tables constructed for an ogival head struck with a radius of *one* diameter and a *half* to one struck with a radius of *two* diameters and to the standard density of air used in France. As a matter of course, I objected very strongly to the Krupp Firm making such a free use, *without the slightest acknowledgment*, of the results of my careful experiments continued through several years. I therefore issued a Protest against their cool proceedings, consisting chiefly of references to my earlier ballistic publications (28).

3. The object of my Protest was to direct the attention of those interested in ballistics to the *prior publication* of the results of my experiments with elongated projectiles 1867–1879, and to my general tables founded thereupon, 1871, 2 and 1879.

4. Mr Fried. Krupp acknowledges that he was the owner of a copy of my first published table, 1871. Also when he signed his "Replica," if not before, he must have known that my tables had been often reprinted, and that they were in common use at Woolwich long before the publication of his tables, 1881. Still, Krupp thinks it sufficient to affirm that *my results were not used*, and that he *made his own* experiments. He concludes his weak defence in 1885, with a ludicrous boast about employing a *perfectly new method* which secures an *exactness never obtained before!*

5. Now, so far as I know, this "perfectly new method" for velocity and space, or S_v, was published by me, for both *spherical* and *elongated* projectiles, in 1871[1]; and for velocity and time, or T_v, in 1872[2]. Copies of both these publications were forwarded to Mr Fried. Krupp in 1885. These tables derive their chief importance from the fact that they present, in a useful form, the *results* of long continued and careful experiments made by me to determine the resistance of the air to the motion of both *spherical* and *elongated* projectiles.

6. As it is probable that this translation may fall into the hands of persons not well acquainted with all particulars of the case, I will give a brief account of the work done at Woolwich 1865–1880, as an introduction to the translation of Mr Fried. Krupp's "Replica," and of the remarks made upon it by the Editor of "Rivista."

7. A careful examination of the experiments made to determine the resistance of the air to the motion of elongated projectiles satisfied me that nothing of any value had been accomplished down to 1864. The ballistic pendulum and Navez chronoscope were at that time the instruments in favour at Woolwich. But they were not at all suitable for the work required to be done.

8. The new chronograph constructed by me at Woolwich, 1864–5, was specially adapted to determine the resistance of the air to the motion of projectiles by recording the times occupied by shot in passing over a *succession* of *equal* spaces. When such an instrument was properly used there could be *absolute* errors

[1] *Remaining Velocity*, &c., 1871.

[2] *Proceedings of the R. A. Institution*, 1872.

only at the *first* and *last* screens, and these errors ought not to exceed the $\frac{1}{2000}$th of a second. If at any intermediate screen there was a small error $\pm \delta t$ on one side, there would be an equal and opposite error $\mp \delta t$ on the other side of that screen. By drawing curves, or by differencing the times between successive screens, small corrections could be introduced to make them perfectly consistent with one another. Now as every record made by an electro-magnet must of necessity be affected by an error, in this case, every care was taken to secure *equality in the errors*, and then their bad effects disappeared entirely. Thus, every screen record was made by the *same* electro-magnet acting on the *same* marker, and to avoid the disturbances of *remaining magnetism*, the contact breaker was kept in action down to the instant of firing the gun. And the clock arranged to give the time records broke its current every second.

9. But this cannot be done with the ordinary chronoscopes, as every record is made by a *different* electro-magnet, and therefore *remaining magnetism* cannot be entirely avoided in the use of those instruments. Thus in measuring a *velocity* by one of these ordinary instruments there are *two independent* errors, and in measuring the *resistance* of the air there are *four independent* errors, for which no test can be applied.

10. When the needful corrections in my records were sufficiently small, the experiment for that round was known to be a *good* one, and must be received as such. For a velocity 1300 f.s. of a 5-inch shell weighing 24 lbs. a variation of 0″·00002 in the value of $\Delta^2 t$ will cause a corresponding variation of 1 in the value of K. Owing to this extreme delicacy of my method of experimenting, any small degree of unsteadiness in the motion of the shot was made manifest. It was not surprising therefore that my coefficients of resistance K varied considerably, for, in *practice*, projectiles fired under the same conditions are known to vary much in the degree of steadiness of their motion.

11. After a very satisfactory trial of the new chronograph with *ten* screens, 1865, it was manifest that the key to the proposed problem had been found[1]. The first set of experiments was made

[1] *Proceedings of the R. A. Inst. Woolwich*, 1866, p. 161, and *Reports*, &c., 1870, pp. 3–9.

at Shoeburyness in 1866, to determine the resistance of the air to elongated projectiles provided with (1) Hemispherical heads; (2) Hemi-spheroidal heads, axes as 1 to 2; (3) Ogival heads struck with a radius of *one* diameter; and (4) Ogival heads struck with a radius of *two* diameters[1]. The diameter of the shot was 4·7 inches.

12. The gun used was a very good M.L. gun. In this respect we were fortunate, for careful experiments had shown that studded shot were more steady in their flight than lead-coated projectiles[2] fired from B.L. guns. This was only what might have been expected, because the soft metal studs partly wore away, thus leaving one facet of the worn stud to slide smoothly along the bore of the gun, while the other facet imparted rotation to the shot. These experiments showed that ogivals struck with a radius of *one* diameter encountered a resistance of the air exceeding by about 6 per cent. that offered to the motion of ogivals struck with a radius of *two* diameters.

13. After the completion of these experiments it was decided by government to carry out an extensive series of experiments with both *spherical* and *elongated* projectiles having ogival heads struck with a radius of one diameter and a half. The bores of the M.L. guns used were 3, 5, 7 and 9 inches. Both solid and hollow projectiles were made use of in these experiments, which were carried out between Oct. 7, 1867 and Nov. 5, 1868. The charges of powder were varied so as to give coefficients for velocities 900—1700 f.s., for *elongated shot.* The Report was dated July 23, 1868[3], and the values of the coefficients for various velocities were first published in 1868[4]. For *spherical* shot, the velocities varied from 850—2150 f.s., and the Report was dated Feb. 13, 1869[5]. About 380 rounds were fired. Afterwards the reduction of the records being continued to *five* places of decimals the coefficients for *elongated* shot were revised[6]. The whole was at once printed and referred Dec. 16, 1868[7] to Sir G. B. Airy, Astronomer Royal,

[1] *Reports*, &c., 1870, pp. 10—26, and *Trans. of the Royal Society*, 1868, pp. 417—441.

[2] *Proceedings of the R. A. Inst. Woolwich*, VII. 87.

[3] *Reports*, &c., p. 18.

[4] *Trans. of the Royal Society*, 1868, p. 441.

[5] *Reports*, &c., p. 55. [6] *Ib.*, p. 123. [7] *Ib.*, p. 153.

to Professor Stokes, Secretary to the Royal Society, and to Professor Adams, Director of the Cambridge Observatory. This reference was suggested by the Committee with whom I acted in carrying out these experiments, as we were anxious that the Secretary of State for War should be correctly informed as to the precise value of our experiments. Instead of the reference being made to one only of the names suggested, as was intended, it was made to all three. From the Report of this Committee of Reference, it appears that other experiments and chronoscopes were brought before the Committee, with a view to influence their judgment, but I was never allowed to see these statements. The Report extended to six pages and discussed the chronoscopes of Navez, Leurs, Benton, Vignotti, Boulengé and Schultz. The Referees finally observed:

"We consider that these experiments of Professor Bashforth are "admirably planned, and that the results obtained are very valuable[1]."

14. Afterwards my spare time was employed in reducing the results obtained by experiment, to forms adapted for general use. In 1871–2 I published my general tables for both *spherical* and *elongated* ogival-headed projectiles, when supposed to move in a straight line under the resistance of the air only[2].

15. From the results of my experiments I was able to announce the following laws in 1871:

"For ogival-headed elongated shot, the resistance of the Air may "be considered to vary roughly as the *sixth* power of the velocity for "velocities 900—1100 f.s.; to vary as the *third* power for velocities "1100—1350 f.s.; and to vary as the *second* power for velocities above "1350 f.s.[3]"

16. And from the value of the coefficients then found, it appeared that $k = 143{\cdot}9$[4]. This law of resistance deduced in 1871 from experiments with velocities 1350—1700 f.s. was found by subsequent experiments to hold good for velocities 1300—2800 f.s., with a slightly reduced coefficient $k = 141{\cdot}2$. This fact is a sufficient proof of the accuracy of the system of experimenting adopted.

[1] *Reports*, &c., p. 160.

[2] *Remaining Vel.*, &c., 1871, p. 47, and *Proc. of the R. A. Inst.*, 1871, 1872.

[3] *Remaining Vel.*, &c., p. 21, and *Proc. of the R. A. Inst.*, 1871, p. 367.

[4] *Remaining Vel.*, &c., p. 33, and *Proc. of the R. A. Inst.*, 1871, p. 377.

The general laws of resistance of the air, first enunciated as above, are those now generally adopted with only trifling variations.

17. I pursued my long and laborious work 1864–1869 in the expectation that my results would be favourably received by scientific men. My reports giving full particulars of my experiments were published from time to time by government, much to my advantage, and all persons interested in the subject of ballistics were perfectly free to use my results, when duly acknowledged, *provided only they did not claim credit for work already done by myself.*

18. At every step I had to encounter the ill-will and opposition of the Ordnance Select Committee. But fortunately, I had the hearty support of the Council of Military Education, of the Director of Artillery Studies, of the Professor of Artillery of the R. M. Academy, and of three distinguished members of the Advanced Class.

19. Most of my would-be rivals have shown remarkable ignorance of, or contempt for, the rules respecting scientific discoveries, which are commonly recognised among scientific men. Some appear to think that it is quite sufficient to produce some *unpublished* result as a satisfactory ground for their claim to *priority*; while others, as Mr Fried. Krupp, write as if they need only say that they had derived their published results from their own experiments to entitle them to rank as discoverers, although the same results had been discovered and published about ten years before. But these second discoverers may rest assured that, when any one has once worked out a subject at the manifest expense of great care and labour, and *published* his satisfactory results, he has established a claim which no wise man, who values his own character, will attempt to disturb.

20. Thus General Mayevski published his "Traité de Balistique Extérieure" in 1872, in which he made very considerable use of my labours in a manner far from satisfactory to me. That writer remarks—"Les résultats des expériences faites par M. Bashforth en Angleterre sur les projectiles *oblongs* ont été déduits des données insérées dans les *Proceedings of the Royal Artillery Institution*, Woolwich, 1868. Les expériences de St-Pétersbourg

sur la résistance de l'air au mouvement des projectiles *sphériques* et *oblongs* ont été faites par nous en 1868 et 1869 et leurs résultats sont pour la *première fois publiés* dans notre traité." Preface vi, 1872.

21. Here Mayevski mentions his own experiments with *spherical* projectiles, 1872, for velocities 227—527 m.s. or 745—1729 f.s., but he is quite silent as respects my own results for *spherical* projectiles reported 1869 for velocities 850—2150 f.s. As my general table founded on these experiments was published in 1871, and Major Ingalls published a similar table, 1885, *said to be founded on* Mayevski's results, 1872, it is easy to compare the results given by the two tables with the results given in the original report, 1869, by calculating by each table the velocity of a 100 lb. spherical projectile at every 1000 feet, supposing it to start with a velocity of 1700 f.s. Here $d^2 \div w = 0{\cdot}7766$.

Distance.	Report 1869.	Bashforth 1869.	Mayevski 1872.	Diff.
0 feet.	1700 f.s.	1700 f.s.	1700 f.s.	0 f.s.
1000 „	1453 „	1452 „	1452 „	0 „
2000 „	1249 „	1248 „	1239 „	9 „
3000 „	1088 „	1087 „	1075 „	12 „
4000 „	969 „	969 „	957 „	12 „
5000 „		878 „	866 „	12 „

This shows clearly that the Mayevski results of experiments with *spherical* projectiles 1872 are *identical* with my own of 1869, excepting one slight break in the velocity about 1400 f.s.

22. With respect to the results of experiments made with *elongated* projectiles, Mayevski gives the following account of his own peculiar proceedings. His experiments with *spherical* shot were said to have been carried out in 1868, "et en 1869 on a exécuté...des expériences sur la résistance de l'air au mouvement des projectiles *oblongs*[1]." "Aussi *pour compléter* les données se rapportant aux projectiles de forts calibres *nous avons profité des tableaux des vitesses décroissantes déduites* par M. Bashforth de ses expériences faites en 1868 au moyen de son chronographe; ces tableaux comprennent les vitesses de 518 m.s. à 283 m.s. ...," or 1700—928 f.s.[2]

[1] *Traité*, p. 37. [2] *Ib.*, p. 38.

23. The coefficients of resistance for *elongated* projectiles derived from my experiments and thus *appropriated* by General Mayevski, according to his own statement, were for velocities 942, 955, 984, 991, 1037, 1050, 1056, 1089, 1096, 1116, 1132, 1165, 1175, 1181, 1375, 1377, 1509, 1667 and 1680 f.s.! quite sufficient to give my own law of resistance for all velocities between 900 to 1700 f.s. At first, I did not raise any objection to the General's proceedings, as he appeared to have acknowledged the use he had made of my labours, for I did not then duly appreciate the full significance of his misleading title given to the results of this discreditable compound—namely "résultats...*russes* et anglaises," pp. 40, 41, 42, thus cunningly taking possession of my results and destroying all connection between my name and my experiments, both for *spherical* and *elongated* projectiles. I have only to state that the English results had been reported to government in 1868, the year before these sorry Russian experiments are said to have been made. I wish to add that I have not, and never had, any connection or partnership with Mayevski or anything Russian. I have never before met with a case where a would-be experimenter has tried to give importance to his results, by helping himself freely, and without leave, to the results of another experimenter, which had been *published and were complete in themselves,* for velocities 900—1700 f.s.

24. Thus I have shown that Mayevski's reported results for *spherical* shot 1872 are entirely in accord with my own *previously published* coefficients (21). Hence it is plain that Mayevski has no claim to any scientific discovery based on his so-called Russian experiments with either *spherical* or *elongated* projectiles, 1868 and 1869, and that the General has done his best to deprive me of the full credit of my own work.

25. A Treatise on the Motion of Projectiles was published by me early in 1873, containing coefficients K of Resistance of the air, and general Tables for both spherical and elongated projectiles. Very extensive tables were calculated to render John Bernoulli's solution of practical utility in the calculation of Trajectories, for the *cubic* law of resistance of the air. In my anxiety to render my work complete, the tabular numbers for the *cubic* law of resistance were carried to *five* places of decimals where *four* places would have

been quite sufficient. These tables gave functions (X) and (Y) to be used in calculating the coordinates of the trajectory, and (T) to be used to find the time of flight. Afterwards the coefficients of resistance of the air to elongated shot were found by experiment for velocities 430—2250 f.s. in 1879; and finally for velocities 100—2800 f.s. in 1880.

26. All the original records of the experiments 1867, 8 were mounted in a scrap-book, and these, with the note-books containing the original reductions of the experiments, have been handed over to the Department of Artillery Studies, Woolwich. The Chronograph is in the South Kensington Museum.

27. Owing to the satisfactory recognition of the value of my ballistic labours by the Right Hon. Lord Hartington when Secretary of State for War 1885, I was led to revise the reduction of all the experimental rounds fired in 1867, 68, 79 and 80 and to calculate new General Tables from the new and slightly modified coefficients for both English and French measures. I further calculated the tables required to render Bernoulli's solution applicable to the *Newtonian* law of resistance. A selection was made from the tables for the *cubic* law published in 1873 when a new column (V) in the tables was added to facilitate the division of the trajectory into arcs suitable for calculation, necessitated by high elevations, and the variation of laws of resistance.

28. My Protest against the proceedings of the Krupp Firm took the form of a list of my early ballistic publications, which were left to speak for themselves, as follows:—

1. Tables of Remaining Velocity, &c. London, 1871.
2. Proceedings of the R.A. Institution, Woolwich, 1871, 2.
3. Mathematical Treatise on the Motion of Projectiles. London, 1873.
4. Government Treatise on the Manufacture and Construction of Ordnance 40185/18. London, 1877.
5. Handbook for Field Service, 1878.
6. Principles of Gunnery, by Major Sladen, R.A., 1879.
7. Manual of Gunnery for H.M. Fleet, 1880.
8. Supplement to Motion of Projectiles. London, 1881.

9. Useful Rules and Tables by Lt.-Col. W. H. Noble, R.A., 1882.
10. Text Book of Gunnery, by Capt. Mackinlay, R.A., 1883.
11. Proceedings of the R.A. Institution, Woolwich, Vol. XII. 1883.

&c. &c.

I might also have referred to Niven on the Calculation of Trajectories, *Proceedings of the Royal Society*, 1877; to Colonel Siacci's Method, 1880, and to the Article "Gunnery" in the *Encyclopædia Britannica*, 1880.

29. Krupp's Tables made their appearance at Essen, 1881. And it is stated that they had been "établi par l'usine Krupp au commencement de l'année 1880," p. 18. But no details have been furnished. No information has been given respecting the instruments used in making the experiments. No reference was made to any prior experimenter. I here give a diagram, fig. 1, showing the resistance of the air, in kilogrammes per square centimetre of cross-section, to an elongated ogival-headed shot struck with a radius of two diameters for various velocities, supposing the weight of a cubic metre of air to be 1·206 kg. The line AA represents

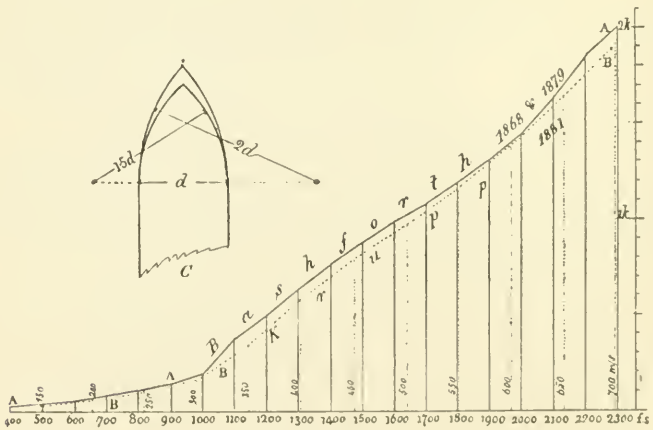

Fig. 1.

the results of my careful experiments, while the dotted line BB represents the law of the resistance used by the Krupp Firm in 1881. C shows the comparative forms of heads struck with a radius of *two* diameters, and of *one* diameter and a *half*.

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ALLA PROTESTA DEL

SIGNOR BASHFORTH.

In the September number the "Rivista di Artiglieria e Genio" published a Protest of Professor Bashforth, in which he charges the Krupp Firm with having made use of his publications concerning the resistance of the Air, *without having mentioned the source.* (*a*).

Mr Bashforth had already communicated with me directly about this same abuse.

Here follows a translation of his letter and of my reply:

HORNCASTLE, *June* 4, 1885.

Mr FRIED. KRUPP,

In acknowledging the receipt of your *three* ballistic pamphlets, I beg your acceptance of *five* of my early publications on the same subject, which will sufficiently explain my grounds for objecting to the publication of your Tables of Remaining Velocities, &c.

(*a*) My Protest gave merely a list of my publications on the resistance of the Air to the motion of projectiles, with dates. It was left open for Mr Fried. Krupp to reply, either (1) that he had published the same results before me (1868), giving reference to date and matter of his publication; or, (2) that he was unacquainted with my work but was ready to do what was right in the matter. In either case there would have been an end of all dispute. But neither of these courses suited Mr Fried. Krupp.

without the slightest reference to the results of my own experiments. (*b*).

(*Signed*) F. Bashforth.

Essen, 8 *July*, 1885.

Professor Bashforth, Horncastle,

For the present I must energetically refute the *charge* which you have made in your letter of the 4th of June of having taken advantage of your labours without acknowledgement. (*c*).

In the tables for the calculation of the final velocity and of the time, *the result of your work was not used at all.* (*d*).

Of the pamphlets sent with your letter of the 4th of June one only was known here, that is to say, the oldest *one of the year* 1871, but *even the results of this pamphlet were not used* (*d*) for the table above mentioned. This you ought to see directly on making a comparison. My table is deduced *exclusively* on the basis of experiments

(*b*) The *three* pamphlets *received* by me were, (1) Krupp'sche Tabelle, &c., Essen, 1881; (2) Der Luftwiderstand, &c., Essen; and (3) Ballistische Formeln von Mayevski nach Siacci, Essen, 1883. The *five* pamphlets I *sent* in return were (1) Tables of Remaining Velocity, &c. 1865–1870, London, 1871; (2) Resistance of the Air to the Motion of Elongated Projectiles having variously formed heads, *Trans. of the Royal Society*, 1868; (3) Proceedings of the R.A. Institution, Woolwich, Jan. 1872; (4) Report of my experiments, 1878–79, Part II., London, 1879; and (5) Final Report of my experiments, 1878–80, London, 1880.

(*c*) My tables, &c., were published 1868–72, for the use and convenience of anyone disposed *to use them in a legitimate manner.* But I could not, without Protest, allow anyone else to come forward as the author of the results on which those Tables were founded.

(*d*) It matters not whether Mr Fried. Krupp did use my labours or not. My results had been *published long before Krupp commenced his labours.* Second discoverers are nobodies.

made with *projectiles and cannons of my own construction* in the Dutch poligon or in my own poligon. (*e*). Even the curve published by you shows that your results are *not valid for my cannons and projectiles.* (*e*).

Besides General Mayevski has already demonstrated as above, that which results from the tables of Siacci, which are reduced on the basis of *your* results and therefore are not of use for *cannons and projectiles of my system* (*e*), because the resistance of the air in that case would have to be much greater than it is in reality.

I expect then that you *recall publicly the charge which* you have raised against me in your protest, otherwise I will take the steps necessary to repel your assertions as *false.* (*f*).

(*Signed*) FRIED. KRUPP.

Mr Bashforth founds his protest exclusively on the presumption that *I might have used his publications*

(1) because the resistance of the air taken for the basis of my tables did *not diverge much* from his own indications, and

(*e*) Not of the slightest importance. My experiments were made 1868 with ogivals struck with a radius of one diameter and a half, while Krupp's were ogivals struck with a radius of two diameters. My tables could easily be adapted to Krupp's case by his own teaching under "Geschosse besonderer Form," p. 22. My experiments shew that a reduction of resistance of 3 per cent. would suffice to adapt my tables to ogivals struck with a radius of two diameters. A further reduction of 1·37 would be required to reduce the English to the French standard density of the air, making in all a reduction of 4·37 per cent. This correction has been applied in the figures 1 and 2 to my results.

(*f*) This polite language of course ended my correspondence with Mr Fried. Krupp. I have not recalled any charge that the contents of my Protest implied—namely that the publication of Krupp's tables was an invasion of my rights as an author. See the remarks of the Editor of the Revista. "Sic vos non vobis."

(2) because my tables, as he affirms, *have appeared since* his publications. (*g*).

Every impartial experimenter will admit in the first place that the coefficients of resistance which result from different experiments must be the same for the given velocity if the calculation *be not mistaken.* (*h*).

Besides whoever is engaged with this object will know that in all the great artillery establishments, as at least in my poligon, they make continually experiments to determine the force of the resistance of the air. (*i*).

Moreover the affirmation of Bashforth concerning the order of time is mistaken, because in reality my table was *finished before that of Bashforth.* (*j*).

The two reasons which Mr Bashforth considers sufficient to charge me with the unlawful use of his own materials would by others be esteemed *hardly sufficient.* Nevertheless to remove all doubt on this point I give the following account of my table for the calculation of the final velocity and of the time.

The Lieutenant-captain of the Dutch Marine Artillery actually Minister of Marine, Mr Gericke communicated to me in the beginning of the year 1878, as the result of his experiments and calculations the following tables (see pp. 21, 22) on the resistance to projectiles of my construction. (*k*). This table was printed in the Dutch

(*g*) Here it is admitted that the two publications did *not diverge* much from one another, but which was the *earlier?*

(*h*) Suppose one or both mistaken as has commonly happened.

(*i*) A vast number of so-called experiments have been made without any definite result.

(*j*) Support this assertion of *priority* with satisfactory evidence and so end all dispute.

(*k*) It appears necessary to reprint these two tables in full, although they are to me perfectly unintelligible standing as they do

Tavola I.

Coefficienti di resistenza dell' aria per granate ordinarie con anelli di rame, di 2,8 calibri di lunghezza.

Velocità in	Valori di K ad uso delle tavole di Bashforth		Valori del Coefficiente b se la resistenza dell' aria è proporzionale alla							
			1ª potenza della velocità		2ª potenza della velocità		3ª potenza della velocità		4ª potenza della velocità	
m	K	ΣK	1^b	$\Sigma\, 1^b$	2^b	$\Sigma\, 2^b$	3^b	$\Sigma\, 3^b$	4^b	$\Sigma\, 4^b$
							0,00		0,00000	
200	41	41,0	49,65	49,65	0,24824	24824	12412	12412	62058	62058
210	42,5	83,5	56,74	106,39	0,27018	51842	12866	25278	61265	123323
220	44	127,5	64,47	170,86	0,29303	81145	13320	38598	60546	183869
230	45,5	173,0	72,87	243,73	0,31680	112825	13774	52372	59887	243756
240	47	220,0	81,95	325,68	0,34148	146973	14228	66600	59284	303040
250	48,5	268,5	91,76	417,44	0,36705	183678	14682	81282	58729	361769
260	50	318,5	102,32	519,76	0,39354	223032	15136	96418	58217	419986
270	51,5	370,0	113,65	633,41	0,42094	265126	15590	112008	57743	477729
280	54	424,0	128,16	761,57	0,45772	310898	16347	128355	58382	536111
290	57	481,0	145,28	906,85	0,50040	360938	17255	145610	59500	595611
300	62	543,0	168,92	1075,77	0,56306	417244	18769	164379	62563	658174
310	69	612,0	200,73	1276,50	0,64753	481997	20888	185267	67381	725555
320	80		247,99	1524,49	0,77497	559494	24219	209486	75681	801236
330	90,5		298,35	1822,84	0,90408	649902	27397	236883	83021	884257
340	97		339,45	2162,29	0,99840	749742	29364	266247	86365	970622
350	100		370,84	2533,13	1,05953	855695	30273	296520	86491	1057113
360	101		396,25	2929,38	1,10070	965765	30575	327095	84931	1142044
370	100,5		416,60	3345,98	1,12595	1078360	30424	357519	82227	1224271
380	99,75		436,03	3782,01	1,14748	1193108	30127	387716	79466	1303737
390	99		455,84	4237,85	1,16883	1309991	29970	417686	76847	1380584
400	98		474,68	4712,53	1,18670	1428661	29667	447353	74168	1454752
410	96,5		491,07	5203,60	1,19773	1548434	29213	476566	71253	1526005
420	95		507,31	5710,91	1,20786	1669220	28758	505324	68473	1594478
430	93,5		523,36	6234,27	1,21710	1790930	28304	533628	65824	1660302
440	92		539,17	6773,44	1,22540	1913470	27850	561478	63296	1723598
450	90,5		554,77	7328,21	1,23286	2036756	27396	588874	60883	1784481
460	89		570,11	7898,32	1,23936	2160692	26943	615817	58570	1843051
470	87,5		585,14	8483,46	1,24496	2285188	26489	642306	56359	1899410
480	86		599,83	9083,29	1,24966	2410154	26035	668341	54239	1953649
490	84,5		614,20	9697,49	1,25346	2535500	25581	693922	52205	2005854
500	83		628,15	10325,64	1,25630	2661130	25126	719048	50252	2056106
510	81,5		641,73	10967,37	1,25829	2786959	24672	743720	48377	2104483
520	80		654,84	11622,21	1,25933	2912892	24218	767938	46573	2151056
530	78,5		667,54	12289,75	1,25950	3038842	23764	791702	44837	2195892

Tavola II.

Coefficienti di resistenza dell' aria per granate di ghisa indurita con anelli di rame, di 2,8 calibri di lunghezza.

Velocità in	Valori di K ad uso delle tavole di Bashforth		Valori del Coefficiente b se la resistenza dell' aria è proporzionale alla							
			1ª potenza della velocità		2ª potenza della velocità		3ª potenza della velocità		4ª potenza della velocità	
m	K	ΣK	b_1	Σb_1	b_2	Σb_2	b_3	Σb_3	b_4	Σb_4
							0,00		0,00000	
200	30	30	36,33	36,33	0,18163	18163	09082	9082	45409	45409
210	32	62	42,72	79,05	0,20344	38507	09687	18769	46130	91539
220	34	96	49,82	128,87	0,22644	61151	10293	29062	46786	138325
230	36	132	57,65	186,52	0,25066	86217	10898	39960	47382	185707
240	38	170	66,26	252,78	0,27608	113825	11504	51464	47931	233638
250	40	210	75,68	328,46	0,30273	144098	12109	63573	48437	282075
260	42	252	85,95	414,41	0,33058		12714	76287	48902	330977
270	44	296	97,10	511,51	0,35963		13320	89607	49333	380310
280	46	342	109,18	620,69	0,38992		13925	103532	49733	430043
290	48	390	122,21	742,90	0,42140		14531	118063	50106	480149
300	50	440	136,23	879,13	0,45409		15136	133199	50454	530603
310	52,5	492,5	152,73	1031,86	0,49269		15893	149092	51269	581872
320	58	550,5	179,80	1211,66	0,56186		17558	166650	54870	636742
330	72	622,5	237,36	1449,02	0,71927		21796	188446	66050	702792
340	90	712,5	314,96		0,92634		27245	215691	80131	782923
350	96,5	809,0	358,69		1,02482		29213	244904	83466	866389
360	97,6	906,6	382,91		1,06365		29549	274453	82074	948463
370	96,75	1003,35	400,96		1,08368		29289	303742	79159	1027622
380	95,85	1099,20	418,99		1,10260		29016	332758	76359	1103981
390	94,9	1194,10	436,96		1,12040		28729	361487	73665	1177646
400	93,9	1288,00	454,82		1,13705		28426	389913	71065	1248711
410	92,85	1380,85	472,49		1,15243		28108	418021	68557	1317268
420	91,75	1472,60	489,96		1,16655		27775	445796	66131	1383399
430	90,60	1563,20	507,13		1,17938		27427	473223	63784	1447183
440	89,40	1652,60	523,95		1,19080		27064	500287	61509	1508692
450	88,15	1740,75	540,38		1,20083		26685	526972	59301	1567993
460	86,85	1827,60	556,34		1,20943		26292	553264	57156	1625149
470	85,50	1913,10	571,77		1,21653		25884	579148	55070	1680219
480	84,10	1997,20	586,59		1,22206		25459	604607	53041	1733260
490	82,65	2079,85	600,74		1,22600		25020	629627	51061	1784321
500	81,15	2161,00	614,16		1,22830		24566	654193	49132	1833453
510	79,6	2240,60	626,76		1,22890		24097	678290	47249	1880502
520	78	2318,6	638,47		1,22783		23612	701902	45409	1926111

periodical "De Militaire Spectator" part 8 of the year 1878. In the first place I have *compared this table* with my own results, and subsequently *I have continued it to* 700 m. (*l*).

The necessary coefficients of the resistance of the air were determined by *myself from my own experiments* (*e*) as well as my reports of shooting prove; No. I. August, 1878; No. VI. December, 1878; No. VII. January, 1879; No. XIII. of November, 1879. (*m*).

In this last report one finds indicated also the manner in which, in inverted order, I have worked out the table for the calculation of the final velocity and of the time.

This table was prepared *exclusively* by *the use of my poligons* (*l*), and *never intended* for publication.

In the first place—towards the end of the year 1879—it existed only in a few written copies. Finally in the middle of May 1880 it was printed after that its *great use* and *convenience* in calculation were demonstrated. (*n*).

without explanation. I do however remark that Captain Gericke has referred to my values of K and ΣK in the first two columns in each table. These tables extend from 200—530 m.s.

(*l*) Krupp compared his table with that of Gericke and then *continued* it to 700 m.s. It is instructive to compare dates and progress made:

Step I.	1873, My Motion of Projectiles published with Tables,	177—518 m.s.
	1878, Captain Gericke published his Tables,	200—530 m.s.
Step II.	1879, My Experiments and Tables extended from	131—686 m.s.
	1881, Krupp extended Gericke's Tables from	140—700 m.s.
Step III.	1880, I extended my Coefficients and Tables to	850 m.s.
	1881, Krupp extended his Experiments to	900 m.s.

It is evident that the Krupp party *followed* me step by step, and these facts afford strong evidence that they did convert the results of my labours to their own use.

(*m*) These reports of shooting have never been seen by me, but I have a copy of Report XXX. [considered hereafter (*q*)], which is perhaps sufficient for all practical purposes.

(*n*) The "great use and convenience" of my general Tables had been demonstrated long before 1879. See Tables of Remaining Velocity

Also this table when printed *was never published* (*o*), but was given solely to the officials, who casually were acquainted with it at Essen or Meppen.

The table then had its origin *exclusively from experiments made with Krupp cannons in the poligons of Dutch Artillery* and in my own poligon near Meppen. (*l*).

At the first, the "granate ordinarie" and those of "ghisa indurita" were treated separately, following in this the example of Captain Gericke. However when we began to use the table, we saw the convenience of reuniting in one only the two tables and of taking into consideration the different forms by means of a coefficient. *In such manner I formed the table of* 1881. (*p*).

While the first table comprised the velocity from 10 to 10 m., the second was regulated from metre to metre, in order to avoid interpolations. For this second table I used solely the first table and *the results obtained at Meppen.* (*e*).

In the year 1881 I have also tested the resistance of the air up to 900 m. of velocity, as my report XXX. demonstrates. (*q*). I would desire *to accentuate* this point

1871; and Proc. of the R.A. Institution, 1871, 2; Handbook for Field Service, 1878, pp. 292-315; Colonel Sladen's Principles of Gunnery, &c., &c.

(*o*) This table in dispute was actually printed, and circulated in the United States, America. What is the meaning of Mr Fried. Krupp in stating that it was never published? Why did he print and circulate both a French and a German edition?

(*p*) I only know Captain Gericke's Tables I. and II. here reprinted. Mr Fried. Krupp has already told us how his tables were composed, see (*l*).

(*q*) I have previously on two occasions noticed publicly the discreditable contents of report XXX. Rounds 7, 8, 9 and 10 were fired July 5, 1881; and the velocity of the shot was measured by a *pair* of Chronoscopes at 30 metres from the gun: by another *pair* at 130 metres; and by a third *pair* at 500 metres from the gun:

already now, to the end that later on no one may throw on me again the *blame of having profited by the labours of others.* (*l*).

Neither has *a publication, properly speaking,* of my table of 1881 taken place (*o*); it was only given to those officials who are employed specially with ballistics.

No one can contend that I have not given with my table for calculating the final velocity and the duration of the trajectory, employing a perfectly new method, a ballistic device which unites the greatest simplicity in use with an exactness never obtained before. (*r*).

(*Signed*) FRIED. KRUPP.

Round	Vel. 30 m. from Gun			Vel. 130 m. from Gun			Vel. 500 m. from Gun
	m.s.	m.s.	m.s.	m.s.	m.s.	m.s.	
7	896·4	and 892·5	diff. 3·9	855·9	and 850·9	diff. 5·0	miss and miss
8	903·8	,, 894·5	,, 9·3	852·7	,, 862·7	,, 10·0	miss ,, miss
9	907·4	,, 887·2	,, 20·2	857·6	,, 856·7	,, 0·9	438·1 ,, miss
10	907·4	,, 911·4	,, 4·0	854·1	,, 834·7	,, 19·4	miss ,, miss
Means	900·1 m.s.			853·2			438·1

These mean velocities were used in calculation and gave $\lambda 10^6 = 3{\cdot}65$; while the value finally adopted was 3·66, so that the experiment might rank as a good one. But the two velocities of each shot measured at 30 metres from the gun differed so much as 13, 31, 66 and 13 f.s.! while those measured at 130 metres from the gun differed 16, 32, 3 and 64 f.s.! where the difference in no case should exceed 1 f.s. And the *solitary* velocity 438·1 m.s. measured at 500 metres from the gun (there being *seven* misses) was adopted as the true mean velocity of all four rounds. Such an experiment was only fit for the waste paper basket, but it passed as very good at Meppen.

(*r*) This wordy "Replica" does not meet the question intended to be raised by my Protest, which depended upon published and dated documents. I did not wish any statement of mine to be received unless it was supported by full and satisfactory evidence. My complaint was that Krupp had, in his Tables in 1881, appropriated the results of my laborious and exact experiments of 1867, 68 and 79 without any acknowledgement. My concern was not so much for the Tables themselves, as for the experimental results, by the help of which they were calculated. What can be the good of Krupp's bare

There are two important statements in Mr Fried. Krupp's "Replica" to which I call special attention, because I know no authority for them whatever.

I.

Di più, l'affermazione del Bashforth concernente la successione di tempo è sbagliata, poichè in realità la *mia tavola era finita* prima di quella del Bashforth.

Moreover the affirmation of Bashforth concerning the order of time is mistaken, because in reality *my table was finished* before that of Bashforth.

I call upon the Krupp Firm either to establish the truth of this statement by satisfactory evidence, or, to withdraw it.

II.

Nessuno potrà contestare che non abbia dato colla mia tavola per calcolare le velocità finali e le durate della trajettoria prendendo una *via perfettamente nuova*, un espediente balistico che unisce la più grande simplicità nell' uso con una *esatezza non ottenuta mai prima.*

No one can contend that I have not given with my table for calculating the final velocity and the duration of the trajectory, employing a *perfectly new method*, a ballistic device which unites the greatest simplicity in use with an *exactness never obtained before.*

Again I have to call upon the Krupp Firm either to establish the truth of this statement, or, to withdraw it.

statement that "in reality my table was finished before that of Bashforth," *i.e.* before 1872, without one tittle of evidence for that statement? And just after, it is confessed that Krupp's attention was first directed to the subject in 1878, by the proceedings of Captain Gericke. The whole is concluded by a ridiculous boast of what Mr Fried. Krupp did in 1881, which had been done by myself in 1871, 2; by Niven in 1877; and by Siacci in 1880.

REMARKS OF THE EDITOR OF THE RIVISTA DI ARTIGLIERIA E GENIO.

A Protest of Mr Bashforth.

"Sic vos non vobis...."

The name of F. Bashforth, professor of Applied Mathematics to the Advanced Class of R.A. Officers, at Woolwich, has been known a long time to artillerists, for his chronograph, for many and accurate researches on the resistance of the air, and for a valuable treatise on ballistics.

The first of his experiments on the resistance of the air came to light in 1870, the last in 1880, and together with the report concerning these Bashforth published a table of coefficients of resistance for any velocity between 100 and 2800 f.s. (30 to 853 metres), and also two general tables connecting velocity and time and velocity and space.

In 1881 Mr Krupp published an analogous work under the title *Table de Krupp pour le calcul des vitesses restantes horizontales et des durées de trajet des projectiles oblongs*, Essen, 1881. This table except for a small diminution of the resistance due to a greater acuteness of the Krupp projectiles, *did not differ essentially from that of Bashforth*, as one can perceive from the diagram annexed which we take from the protest.

Of this Mr Bashforth complains, and this does not seem unreasonable, that in the explanations which accompany the Krupp tables there is *perfect silence* concerning the *English* experiments. In this not only does every mention of these experiments fail, but also all information respecting the experimental foundation of the tables. One only reads "Un tel tableau pour différences de vitesse de 10 *m* a été établi par l'usine Krupp au commencement de l'année 1880. Les vitesses restantes calculées d'après ce tableau concordaient très bien avec les vitesses restantes mesurées à Meppen et d'ailleurs. Pour la plus grande commodité du calcul

ce tableau fut changé pour intervalles de vitesse d'un mètre, en même temps il fut corrigé sur la base des résultats des expériences" (p. 18).

From these words we cannot deduce that the experiments of Krupp are the basis of his table. It would appear instead that the Krupp experiments have only served to correct the first table of 1880. Of this table we shall say more later on, but here it is important to note that the date 1880, although it is coincident with the last publication of Bashforth, *could not take away in any* case the priority, because in 1879 he had already published like tables for velocity comprised within 131 and 685 m., which are within a little the limits of the Krupp table (140 and 700 m.). *The priority of Mr Bashforth is therefore indisputable.*

Apart from the question of priority solved from the fact of the dates, it is possible that the tables of Krupp may be independent of those of Bashforth; we, for our part believe that they, at least those of 1880, have been deduced on the basis of experiments executed in that establishment[1].

We have no more under our eyes the original table of Krupp and hence we could not say whether it gives the information which is wanting in that of 1881; but that table reproduced in a work of Mayevski, presents differences sufficiently remarkable from that of 1881, and distinguishes moreover the projectiles of "ghisa ordinaria" from those of "ghisa indurata."

Now this distinction and above all these differences demonstrate a work quite distinct from that of Bashforth. The silence consequently of Krupp respecting Bashforth might be legitimate in 1880, but not in 1881, when the two tables were reduced to one, which, if it does not coincide exactly with the results of Bashforth, confirms them certainly, and confirms them precisely with the diminution which we have pointed out, the silence of Krupp appears to us, we do not say illegitimate, but little reconcilable with his ordinary and well known courtesy.

One does not doubt that in the mind of Krupp there was never the idea of substituting his own name for that of the illustrious English man of Science, and that in that publication, as in

[1] I know nothing of Krupp's experiments of 1880. If they gave results *different* from my own, I have nothing to say. I only object to the Krupp firm posing as discoverers of results *previously published* by me.

other later ones he had the sole design to render service to Artillerists. And Artillerists are thankful to him for it, but they ought also to understand the complaint of the scientific man who has spent 15 years in an important research and *sees afterwards denied him the just as well as noble satisfaction*—sole reward which the scientific man aspires to—*of seeing his discoveries associated with his name.*

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30. I must now notice the Ballistische Formeln von Mayevski Essen, 1883. These "Formeln" appear to be founded upon "Expressions de la résistance de l'air d'après les expériences de Woolwich (1865—1870), de Saint-Pétersbourg (1868), et de Meppen 1881..."[1]. Now my experiments (1865—1870) were made for velocities 900—1700 f.s.; and were in 1879 extended to velocities 430—2250 f.s.; and finally in 1880 to velocities 100—2800 f.s. Mayevski states clearly that his experiments with *elongated* shot were made not in 1868, but in 1869, and not published till 1872, when according to his own account (23) he appropriated, without my leave, the results of my early experiments, 1865—1870. I know nothing of Krupp's experiments at Meppen 1881 beyond those already noticed (note *p*), which are of no value. Thus it appears that my own results have been made

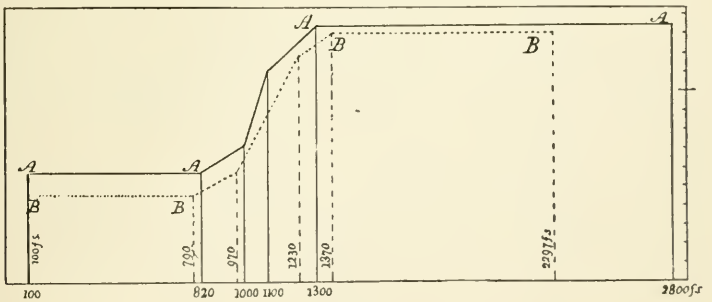

Fig. 2.

the basis of these "Formeln" of Mayevski printed by Krupp, 1883; but without the slightest acknowledgement of the value of the help derived from my labours. Mayevski and Krupp do not appear to agree with the editor of "Rivista" as to the high

[1] *Revue d'Artillerie*, Vol. XXII., April, 1883.

satisfaction an experimenter, who has given several years to his work, feels, on "seeing his discoveries associated with his name." I give in Fig. 2 a diagram AA showing the coefficients of v^2 for my results, and the dotted line BB for those adopted by Krupp and Mayevski[1] in their "Formeln" 1883. The resistances for high velocities differ little, while for lower velocities the Mayevski and Krupp resistances are *decidedly* too low. As there must always be a difficulty in measuring the velocities at two or more points of a projectile moving with a *low* velocity, I was careful to test the value of my coefficients of resistance for *low* velocities by using them to calculate ranges, &c. The Allgemeine Schuss-Tafeln, Berlin, 1879, fell in my way, and I used 70 rounds fired at elevations 5° to 30° with m.v. 300 to 700 f.s.[2] to test my results and found my coefficients rather *too low*. Much more then are the Mayevski coefficients *too low*, as shown in the diagram.

"Defacing first, then claiming as his own."

Concluding Remarks.

31. My experiments made to determine the resistance of the air to the motion of projectiles were for the most part carried out with rounds fired at a *low* elevation, so as to give ranges of 600 or 700 yards. Hence the *elongated* shot would move nearly in the direction of their axes and thus would be subjected to the least possible resistance from the air. But when these projectiles are fired at rather high elevations, they are subject to "drift" and therefore will travel with their axes more or less inclined to the direction of their motion. Consequently the coefficients of resistance, determined as they have been in my experiments, might have been expected to be rather *too low* for calculating range tables. On the contrary, it was found that ranges calculated by my coefficients of resistance fell short of those given in range tables. It was then suggested that I should *reduce* my resistances. But I could not tamper with coefficients determined by me, in the most careful manner, by very precise experiment. I was however quite ready to test my results by the use of any *new* gun.

[1] Ingall's *Exterior Ballistics*, p. 28.

[2] *Final Report*, 1880, pp. 46 and 47.

32. The gun officially chosen was a very good 4-inch B.L. gun, Marks II. and III., and with it was furnished a most admirable Range Table, extending to an elevation of 20°, and a range of 7700 yards. $w = 25$ lbs., $d = 4$ inches, and $m.v. = 1900$ f.s. The head of the shot was ogival struck with a radius of two diameters, for which a reduction of 3 per cent., given by my experiments of 1866, was allowed in my coefficients. In the first place the times of flight were calculated by my General Tables for low elevations, as follows :

Exper. Ranges	1000 yds.	2000 yds.	3000 yds.	4000 yds.
Elevation + 6′	0° 55′	2° 17′	4° 10′	6° 27′
Hor. m. vel.	1899·76 f.s.	1898·49 f.s.	1894·98 f.s.	1887·97 f.s.
Cal. hor. stri. vel.	1455·5 f.s.	1124·9 f.s.	955·44 f.s.	843·4 fs.
Exp. time of flight	1″·80	4″·21	7″·20	10″·49
Calc. „ „	1″·807	4″·170	7″·099	10″·48
Difference	+ 0·007	− 0·040	− 0·101	− 0·01

So that the coefficients of resistance seem rather too low.

Captain May, R.N., has also given a carefully prepared Range Table up to a range of 4000 yards[1]. Here $d = 12$ inches, $w = 714$ lbs., $m.v. = 1892$ f.s.

Exper. Ranges	1000 yds.	2000 yds.	3000 yds.	4000 yds.
Elevation + 6′	0° 50′	1° 44′	2° 46′	3° 56′
Hor. m. vel.	1891·8 f.s.	1891·14 f.s.	1889·79 f.s.	1887·54 f.s.
Cal. hor. stri. vel.	1739·15 f.s.	1593·44 f.s.	1457·74 f.s.	1332·10 f.s.
Exp. time of flight	1″·66	3″·47	5″·44	7″·61
Calc. „ „	1″·654	3″·457	5″·428	7″·591
Difference	− 0·006	− 0·013	− 0·012	− 0·019

Again the coefficients are rather too low. But, the trajectory being slightly curved, the shot would pass over distances slightly in excess of 1000, 2000, &c., yards.

33. Hence it appears that my General Tables connecting velocity and space, and velocity and time were perfectly correct for velocities 1900 to 850 f.s. when tried by recent guns. Attempts were made to measure, by the same chronograph, the muzzle velocity and the time over a long range. It was however

[1] *Proc. of the R. A. Inst.*, Vol. XIV., p. 353.

found difficult to measure the muzzle velocity correctly for the high elevations required to obtain long ranges. The experiments made were satisfactory so far as they went, but the range table furnished with the gun was found to afford a much better test of my coefficients, and this test had the additional recommendation that it could be applied to my results by anyone for his own satisfaction.

34. It appeared to me that Captain May, R.N., was right in his suggestion that for high elevations and velocities, the "kite-like action[1]," or the vertical drift, was the cause of discrepancies in the calculation of long ranges. For owing to the rapid rotation imparted by a rifled gun to an elongated projectile, that projectile will at first tend to maintain the parallelism of its axis. Consequently the point of the projectile will soon rise above the trajectory. The resistance of the air then pressing harder on the lower than on the upper side will raise the projectile bodily upwards, and cause it to move as if it had been projected with a *slightly increased* elevation. Also the resistance of the air tending to raise up the point of the projectile spinning about its axis will, according to Magnus, cause the axis of the shot to describe a conical surface about the moving tangent to the trajectory—the point of the projectile turning to the right when its rotation is right-handed. This rise of the shot is the beginning of all drift. As the axis of the shot moves round the tangent to the trajectory, the resistance of the air is driving the centre of gravity of the shot at any moment from the tangent to the trajectory in a plane passing through that tangent and the axis of the projectile, and this accounts for the drift of the projectile. When the motion of the projectile is steady the vertical angle of the conical surface described by the axis about the tangent to the trajectory must be very small.

35. Under these circumstances the following method has been employed to test my coefficients of resistance for high elevations and velocities, using the range table of the gun chosen by government, for that purpose. Here $d = 4$ inches, $w = 25$ lbs, and $m.v. = 1900$ f.s. A reduction of the coefficients of 3 per cent.

[1] *Proc. of the R. A. Institution*, Vol. XIV. p. 369.

was allowed for ogivals struck with a radius of *two* diameters. Take any elevation α, and calculate r the range, and t the time of flight carefully. Then refer to the range table and find from it, corresponding to the calculated range r, the elevation α' and the time of flight t'. If then $t = t'$, the coefficients are satisfactory and $\alpha - \alpha'$ is the sum of the "jump" and of the vertical drift.

36. This method was most carefully applied in connection with the given range table, making $\alpha = 1°, 2°, 3° \ldots 20°$ in succession. The results of calculation are given in columns 1, 2, 3, 4 and 5 of the following table. The range table furnishes the elevations and times of flight in columns 6 and 7 which correspond to the ranges given in column 1. The difference of columns 2 and 6 give the sum of "jump" and vertical drift in column 8.

Range	By Calculation				By Range Table		Difference of	
1	2	3	4	5	6	7	8	9
Yards r	Elevation α	Time of Flight t	Angle of Descent	Remaining Velocity f. s.	Elevation α'	Time of Flight t'	Elevations $\alpha - \alpha'$	Times of Flight $t - t'$
	°	″	° ′		° ′	″	′	″
1053	1	1·92	1 10	1440	0 52	1·91	8	+0·01
1832	2	3·72	2 44	1175	1 56	3·77	4	−0·05
2433	3	5·36	4 29	1034	2 58	5·44	2	−0·08
2937	4	6·86	6 20	973	3 56	6·99	4	−0·13
3386	5	8·31	8 0	923	4 53	8·41	7	−0·10
3797	6	9·64	9 39	891	5 51	9·77	9	−0·13
4148	7	10·95	11 31	850	6 43	11·05	17	−0·10
4467	8	12·21	13 25	817	7 34	12·23	26	−0·02
4813	9	13·46	15 2	800	8 37	13·57	23	−0·11
5110	10	14·66	16 52	781	9 32	14·74	28	−0·08
5384	11	15·84	18 35	765	10 28	15·84	32	0·00
5664	12	17·01	20 22	748	11 25	16·96	35	+0·05
5924	13	18·14	22 8	734	12 19	18·10	41	+0·04
6170	14	19·29	23 53	723	13 12	19·18	48	+0·11
6398	15	20·40	25 42	712	14 5	20·29	55	+0·11
6632	16	21·53	27 28	705	15 0	21·46	60	+0·07
6821	17	22·58	29 10	697	15 45	22·51	75	+0·07
7003	18	23·62	30 47	691	16 33	23·52	87	+0·10
7221	19	24·75	32 36	686	17 33	24·71	87	+0·04
7483	20	25·94	34 17	684	18 47	26·10	73	−0·16

And the differences of the *calculated* and *range* times of flight are given in column 9. These are so small that it is plain my coefficients are perfectly satisfactory for the best English guns of 1881.

This method of proceeding also gives with great accuracy the angle of descent in column 4, and the striking velocity in column 5.

37. When it was first found that the calculated ranges fell short of the experimental ranges, it was at once concluded that the coefficients of resistance were too high. But when they were reduced to obtain the required *range,* it was found that the calculated and experimental *times* of flight did not agree. Consequently that mode of "correcting" was a failure. The above table shows that when a proper correction of the elevation is made, the result is thoroughly satisfactory. And further we can see the necessity for this correction of the elevation, on account of "jump" and "kite-like action."

38. Having recently published a historical account of ballistic experiments connected with the Advanced Class of R.A. Officers 1864–1890[1], it is not necessary for me now to enter into particulars respecting the criticisms and opposition I had to contend against. The results of my experiments have been extensively used in government books of Instruction, as for example in the *Text Book on Gunnery,* 1879, 1883, 1887, 1897; for the Navy, 1880, &c.; for the Army (Small Arms) 1880 and 1888; for the *Handbook for Field Service,* 1878, &c. This is all so far satisfactory. But a cry was raised that my work was not "practical." On the other hand I maintain that my results were and are of the highest practical importance. What else induced the Krupp Firm to issue both French and German Tables in 1881, to all intents and purposes the same as my own of 1871, 2? Fig. 1. Why have Mayevski and Krupp joined in the publication of "Formeln" in 1883, founded upon my results 1868, &c.? Fig. 2.

39. When the Report on our experiments of 1868 was referred to a Committee for their opinion of the value of our results, the *third* question proposed to them by the Director General of Ordnance was:

"(3) Whether the instrument devised and perfected by that gentleman for recording successive small intervals of time is susceptible of general employment at schools of instruction in gunnery[2]."

[1] *Supp. to a Revised Account,* &c., 1895. [2] *Reports,* &c., p. 153.

Sir G. B. Airy replied: "... I have no doubt whatever that the Bashforth instrument is far superior to the others[1]," and the Committee remarked "We are therefore decidedly of opinion that, for the purpose of determining several velocities of the same projectile, and thence deducing the resistance of the Air, Professor Bashforth's instrument is to be preferred to the other[2]." Yet my Chronograph, although borrowed on two occasions, has been carefully excluded from the Service down to the present time.

40. We have seen that this Chronograph has given with great accuracy the average resistance of the Air to elongated projectiles (36) so that little more work of that kind remains to be done. But, conversely, this chronograph is capable of rendering a still further service to the nation in *testing the steadiness* of the projectiles fired from all *new* guns. For that purpose I have already given an estimate of £350 as the cost of Chronograph, small office, screens, &c., for a range of 500 or 600 yards, or say £500 for a range of 1000 yards. The expense of thus testing the shooting quality of a gun, would be the cost of five to ten rounds of ammunition. Simultaneously target practice might be carried on. There must be no sensible wind.

41. I know nothing whatever about the manufacture of *new* guns, excepting what is published, concerning their defects and that is sufficient to show that a rigid and reliable system of *testing* is absolutely necessary.

42. The Advanced Class of Royal Artillery Officers was established in 1864, with a view to prepare them to fill various positions as Instructors, Heads of Departments, &c. But about 1871 officers who had undergone no previous scientific training received appointments, to the exclusion of those who had had two years of severe training. Consequently when a new class should have been assembled in 1872 there was not a sufficient number of candidates. This led to the appointment of a committee whose report [C. 598] was dated March 1872, which is full of instruction. In 1884 a select committee was appointed to inquire into the working of the Transport and Commissariat Services during the Egyptian Campaign, when strange things were brought to light.

[1] *Reports*, &c., p. 154. [2] *Ib.*, p. 158.

43. Admiral R. A. E. Scott criticised severely the performance of the then recent guns[1]. It appears that then the system of "holding fast" or impeding the initial motion of the shot was the rule. The banded projectiles were said to issue from the 110-ton gun with a very *unsteady* motion. The 9·2-inch 22-ton wire gun, from which the Jubilee rounds were fired, gave its first range 14,691 yards for an elevation of 40°, the flight of the projectile being "very unsteady." Augmenting strips were then hammered between the projectile's bands for the next round when the estimated range was about one half more than before. The eighth round was "unsteady," and one round at 4° and one at 20° were recorded as "very unsteady."

44. A Royal Commission was appointed in 1886 to inquire into the system under which stores, &c., were obtained for Her Majesty's Service [C. 5062]. The proceedings of the House of Commons, Aug. 3 or 4, 1887, afforded a thorough exposure of the system on which most important matters were carried out, and much fault was found with those responsible for the quality of swords, &c., provided for those engaged to fight our battles.

45. But Mr Brodrick was most severe March 25, 1898, saying, "The House would remember into what discredit the Ordnance Factories fell some twelve years ago, or so."..."The old plan was, that the same man chose the weapon, was responsible for its manufacture, and afterwards *tested* it; and under it, the Ordnance Factory was a bye word for backwardness, extravagance and unreliability!"

46. Mr Brodrick speaks highly of the advantages which have resulted from the changes made twelve years ago in the management of the Arsenal. But still it seems very desirable that the steadiness of the shot fired from all Service guns should be carefully tested. The wisdom or otherwise of impeding the initial motion of projectiles in heavy guns should be inquired into. Also the system of rifling requires careful consideration, for the failure of big guns near the muzzle seems to indicate that the adopted system of rifling has caused too much strain on the weakest part of the gun. Let it never again be said that any official advisers

[1] *Morning Post*, Nov. 9, 1889.

of the Secretary of State for War are "protected from all responsibility by the position in which they stand."

47. During the last thirty years, from time to time, my experimental results have been opposed, disputed, corrected, tested, appropriated, but still they remain much as they were, for this reason—my method of experimenting was infallible. When the Chronograph gave eight to ten *consistent* records for one round, there could be no possible doubt of the perfect trustworthiness of the experiment, and the values of K derived therefrom must be accepted. The value of K so obtained, on being compared with the average value of K derived from other rounds for the same velocity, would show the comparative value of that particular round. And the value of the final result must depend upon the quality of the guns used in the experiments, which were, in my case (1868) M.L. guns, and this was a piece of good fortune, for about 1865, during the Armstrong and Whitworth competition, the shooting of various M.L. and B.L. guns was carefully compared, and Captain V. D. Majendie, R.A., has stated that, "the remarkable accuracy of the B.L. Armstrong guns has not been disputed, and yet we find that whenever these *breech-loaders* have been carefully compared for accuracy with *muzzle-loaders*, the result has *invariably* been *favourable to the latter*[1]." Again my results were tested by the help of a very good recent 4-inch B.L. gun of 1881, as already explained (32—36), and the agreement was found perfectly satisfactory. As I am not expecting any great improvement to be made in the steadiness of the projectiles fired from new B.L. guns, I must now consider the question closed, so far as I am concerned. I trust therefore that those, who are satisfied with my results will use them, making the customary acknowledgment, and that those who are not satisfied will make their own experiments *de novo* and publish them.

48. The Jubilee rounds have been the cause of much needless trouble, which would have been materially lessened if proper precautions had been taken. In the first place the gun itself should have been carefully *tested* before it was used to *test* the results of other people (43). In the next place, ballistic experi-

[1] *Proc. of the R. A. Institution*, VII. 87.

ments of precision, if they are to have any value, must of necessity, in the first place, be made when there is very little or no wind. In making my calculations I used tabular values of K, 1881, taking $d = 9{\cdot}2$ inches; $w = 380$ lbs; m.v. = 2360 f.s. For an elevation of 40°, the time of flight on a horizontal plane passing through the muzzle of the gun was found by calculation to be 62″·2, and the range 19,436 yards. The Ordnance Committee found by experiment, with the help of a wind, of force 5—7, blowing obliquely down the range, time of flight 62″·62, and range 21,200 yards. Allowing for the difference of level of the muzzle and of the point struck by the shot, the calculated time is very satisfactory; but the calculated and experimental ranges differed by 1767 yards! On another occasion two more rounds were fired with a wind of force 3 blowing obliquely down the range. The mean range found was now 20,220 yards, giving a reduction of nearly 1000 yards from the former experimental range, but still leaving the calculated range 784 yards less than the experimental range. We may suppose the favourable wind of force 3 responsible for 480 yards, leaving 300 yards to be accounted for. Now I believe the shot was ogival of *two* diameters, while coefficients for one diameter and a half were used in calculation. The actual m.v. was 2375 f.s. instead of 2360 f.s. used in calculation. Nothing was allowed in calculation for "jump" or "kite-like action." The barometer stood at 29·5 inches for the experiment and at 30 inches for calculation. If these things were properly taken account of, they would considerably add to the calculated range. But it was not possible to do so in this case, as the calculation had to be made and sent in before the experiment was carried out.

49. A wind blowing in a horizontal direction will have little influence on the *time* of flight of a projectile, but it will produce an effect on the *range* according to its strength and direction. A knowledge of the force of the wind at the surface of the earth is only a poor guide to finding the strength of a gale at a height of one, two or three miles. In Paris it was found that when the wind moved at the rate of 5 miles an hour at a height of 66 feet, it was moving with a velocity of 16 miles an hour at a height of 994 feet. It would have been instructive if an experiment could have been made with a wind 5—7 blowing up the range. But it

is much to be regretted that the Jubilee experiments were not made in the first instance in perfectly calm weather. Afterwards the effects of moderate winds might have been examined.

50. The values of K, my coefficient for the cubic law of resistance, were determined for elongated projectiles, provided with ogival heads struck with a radius of one diameter and a half for velocities 900—1700 f.s. in 1868 by the use of muzzle-loading guns. But breech-loaders were used to extend the values of K for velocities 430—2250 f.s. in 1879; and to velocities 100—2800 f.s. in 1880. And finally the reductions of all the experiments were revised and published in 1890. The following table will show how slight were the variations introduced in the values of K since 1868, and it has been shown already (10) how trifling would be the effects of these variations. I finally adopt the coefficients of 1890.

v.	K	K	K	v.	K	K
f. s.	1873	1881	1890	f. s.	1881	1890
900	64·4	75·0	73·6	1900	72·1	72·2
1000	75·0	75·0	73·6	2000	68·8	68·5
1100	106·0	106·9	106·9	2100	67·8	67·0
1200	108·9	109·6	109·6	2200	67·0	65·8
1300	107·9	108·6	109·4	2300	65·5	62·2
1400	104·0	104·7	105·2	2400	59·6	57·0
1500	97·2	97·9	98·4	2500	54·4	52·9
1600	89·0	89·7	90·5	2600	52·4	51·5
1700	83·9	83·0	83·5	2700	52·1	51·3
1800	——	77·3	77·4	2800	52·0	51·2

There are well known and simple methods for adapting these coefficients for slight variations of steadiness, form of shot, etc. The trifling changes introduced by the Krupp Firm as shown in Fig. 1, and by Mayevski and Krupp, as shown in Fig. 2, did not call for their interference. The care they both have taken to suppress the mention of my name is remarkable. It is however an easy matter to experiment when the result to be arrived at is known beforehand. My opponents remind me of Columbus and the egg—everybody could make his egg stand on end—when shown how.

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